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AUTHOR Brown, Lynne Harrington
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ABSTRACT

Among the many problems faced by family researchers in conducting experimental research in field settings is that posed by nonequivalent and intact treatment and control groups. Nonequivalence refers to the assignment of individuals to treatments on a nonrandom basis, thus generating treatment groups with different expected values on one or more relevant variables. Treatment groups formed independently of either experimenter or experiment may be viewed as both nonequivalent and intact. Given that practitioners and researcher increasingly view treatment efficacy as an important goal in treatment delivery, the question arises as to how variations in outcomes due to the treatments of prevention programs may be distinguished from variations in treatment outcomes due to nonrandom assignment. The use of an indirect or statistical control technique, the analysis of covariance (ANCOVA), in place of or in addition to experimental control is examined. An overview and explanation of the ANCOVA includes a demonstration of the procedure using outcome data from intact groups. Possible applications of ANCOVA for family studies research with illustrative examples from current literature are presented. This paper shows the utility of the basic ANCOVA model as a statistical control in a broad range of research settings. (CM)

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WHEN GROUPS AREN'T RANDOM:

USING THE ANALYSIS OF COVARIANCE IN FAMILY STUDIES RESEARCH

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by

Lynne Harrington Brown*
Purdue University

*Assistant Professor and Resident Methodologist, Department of Child Development and Family Studies, Purdue University.

Among the many problems faced by family researchers in conducting experimental research in field settings is that posed by nonequivalent and intact treatment and control groups. Nonequivalence as used here refers to the assignment of individuals to treatments on a nonrandom basis thus generating treatment groups with different expected values on one or more relevent variables. If treatment groups have formed independently of either experimenter or experiment they may be viewed as both nonequivalent and intact. Usually thought of in conjunction with educational contexts, such as class membership in a given school, nonequivalent and intact groups are neverthless a frequent occurrance for researchers engaged in the design and evaluation of intervention programs.[1].

Common to most intervention programs is the assignment of groups to treatments wherein group membership is a function of some particular personal or behavioral characteristic. For example, once identified as a potential child abuser, it is not unlikely that parents who were themselves victims of maltreatment as children, will be perceived as belonging to a higher risk group and hence the recipient of a different intervention treatment than parents who were not themselves abused as children (for example see Spenetta and Regler, 1972; Garbarieno and Sherman, 1980). This nonrandom assignment of groups to treatments virtually ensures that highly significant mean outcome differences will be found among the treatment groups, independently of any given treatment. As even a cursory review of the current family studies literature pertaining to intervention programs reveals, researchers are generally aware of the methodological difficulties associated with nonrandom assignment of subjects to treatments (Gray & Wandersman, 1980; LeBow, 1981). However with some exceptions (Avery, et.al., 1981; Pinsker & Geoffrey, 1981; Laosa, 1980), rarely are the methods used to control the effects of nonrandom assignment reported. Rather, vague phrases like "groups were examined, equated, matched...for differences" abound, casting doubts on the validity of the reported findings. Given that practicioners as well as researchers increasingly view treatment efficacy as important a goal as treatment delivery, (LeBow, 1981) the question arises as to how variations in outcomes due to the treatments of prevention programs may be distinguished from variations in treatment outcomes due to nonrandom

assignment. A partial answer and the focus of this paper is the use of an indirect or statistical control technique, the analyses of covarience in place of or in addition to, experimental control.

The fundamental purpose of experimental design regardless of setting or treatment objective is to divide the world into two classes of events: independent variables which are thought to systematically influence the dependent variable and the remainder of the universe, the influence of which is collectively attributed to "random error". In general, precision is achieved by maximizing the influence of variables which can be controlled and minimizing the influence of those that cannot. Control may be of either two types. Direct control which is excercised through the various classes of experimental designs is achieved when the independent variables are under the control of the experimenter and subjects can be randomly assigned to treatment groups. Indirect or statistical control is excercised when random assignment of subjects is not possible, thus resulting in independent variables or variates which are correlated with the dependent variable and which differ from group to group. Statistical control is achieved by adjusting the dependent variable for the effects of the uncontrolled independent variables. The objective of direct and indirect control therefore, is to allow the experimenter to distinguish between variations in treatment outcomes due to treatments from variations in treatment outcomes due to any other causes. To the extent that these "other causes" such as nonrandom assignment can be identified and controlled for, this distinction will be maximized.

Although there are a number of statistical control techniques for assessing the effects of treatments independently of the selection-process employed, none appear more broadly useful to family researchers in general and those involved with intervention programs in particular than the analysis of covarience.[2.] Despite its relatively infrequent use which apparently is largely attributable to an overly inclusive condemnation by psychometricians (Kahneman, 1965; Lord, 1960; Lubin, 1965), there is nothing intrinsically inappropriate about the analyses of covarience proceedures either conceptually or computationally for field research designs. However, as with all statistical techniques, as the complexity of the problem addressed increases, so do the limitations under which this

proceedure may be properly used. Therefore a thorough understanding of both the problem at hand and the analysis of covarience (ANCOVA) is necessary for its successful application.

In general this paper will be organized as follows: an overview and explanation of the ANCOVA will be presented including a demonstration of the proceedure using outcome data from intact groups. This will be followed by detailed discussion of the possible applications of ANCOVA for Family Studies research with illustrative examples drawn from current literature. A discussion of the implications of the application of ANCOVA for future family studies will conclude this presentation.

AN OVERVIEW OF ANALYSIS OF COVARIENCE

In an ideal research world, subjects would be randomly assigned to groups and groups randomly assigned to treatments thus precluding the need for statistical control. In fact a number of social scientists have asserted that unless research can be conducted in this manner, its products are of little value (Cochran, 1957; Lord, 1967; Cochran, 1972; and Jacobson, 1978). It is perhaps the ability to empirically test many of the assumptions underlying classical experimental design via high speed computers that has had the greatest importance in moving research from the ideal conditions of the laboratory to the far-from-ideal conditions of field settings. The use of the analyses of covarience is a case in point.

When the ideal of random assignment can be realized, the standard analyses of varience model for the data of a simple experiment can be represented as follows: $Y_{ij} = \mu + \tau_j + \varepsilon_{ij}$ (1) where Y_{ij} is the score of subjects i under the treatment j. μ is the grand mean of the dependent variable, τ_j is the treatment effect of treatment j; and ε_{ij} is the random error component associated with each observation. Errors are assumed to be normally and independently distributed with a mean of zero and homogenety of varience, ie., $\varepsilon_{ij} = NID\ (0, 0_{\varepsilon}^2)$. A fundamental assumption of this model is that randomization has completely eliminated all differences among subjects in the treatment groups prior

to the administering of any treatment. Consequently, any observed differences in treatment responses among groups can only be attributed to treatments and random error.

However, whenever the researcher by choice or necessity works with intact, nonequivalent groups of subjects such as those common in intervention programs, differences among groups may exist for the dependent variable prior to administration of treatments. Because the goal is to assess differences among groups after treatments are administered, the problem becomes one of removing any pre-existing differences due to nonrandom assignment of groups from the post-treatment scores. In a simple pretest/posttest design for example, for treatment effects to be unequivocally assessed, any variation in the difference between the pretreatment and posttreatment test scores due to inital group differences on the pre-treatment test scores must be removed. More generally, if x_{ij} is a variable on which treatment groups differ due to nonrandom assignment and is a covariate or covaries with the dependent variable, Y_{ij}, then one appropriate model for the data would be the analyses of covariance model:

$$Y_{ij} = \mu + \tau_j + \beta(x_{ij} - \bar{x}) + \varepsilon_{ij} \qquad (2)$$

where μ_{ij} and ε_{ij} are defined in exactly the same manner as the ANOVA model in equation (1). β is the within treatment common regression coefficent for Y on x_{ij}; x_{ij} is the covariate (pretreatment) score for subject i under treatment j, $\bar{x}$ is the grand mean of the covariate or pretreatment test scores.

In words, this model states that the dependent variable "depends" not only upon a treatment effect and a random error component but is also linearly dependent upon the magnitude of the differences in the pretreatment test scores or covariate. If equation (2) is now rewritten to:

$$Y_{ij} - \beta(x_{ij} - \bar{x}) = \mu + \tau_j + \varepsilon_{ij}$$

it becomes obvious that when the linear dependence of the covariate is removed $\beta(x_{ij} - \bar{x})$ from the dependent variable, any differences between the pretreatment test scores and the posttreatment test scores may be attributed to treatment effects and random error. That is, if no variables are related to the dependent variable $\beta = 0$ other than the treatments, no

adjustment is needed because the ANCOVA model becomes identical to the ANOVA model in equation (1).

Equation 3 also makes clear the two-step nature of the basic ANCOVA procedure. In the first step, a linear regression equation is used to predict the dependent variable ($\hat{Y}_{ij}$) or posttreatment scores from the covariate (x_{ij}), the pretreatment test scores. The predicted value of Y_{ij}, $\hat{Y}_{ij}$ is that component of Y_{ij}, the dependent variable which is linearly dependent upon the covariate. The residual $Y_{ij} - \hat{Y}_{ij}$ is that component of the dependent variable which is linearly <u>independent</u> of the covariate. This is because $Y_{ij} - \hat{Y}_{ij}$ in the ANOVA model is identical to $Y_{ij} - \beta(x_{ij} - \bar{x})$ in the ANCOVA model. In the second step, this residual or that part of the treatment effect not due to pretreatment differences, is then analysed using standard ANOVA proceedures. Thus the ANCOVA is really an ANOVA of that component of the dependent variable which is unrelated to the covariate or differences in the pretreatment test scores due to nonequivalence of intact groups. Under most conditions the ANCOVA will provide unbiased estimates of the treatment effects.

Figure 1 About Here

Figure 1 presents a graphical representation of ANCOVA for a two-group experiment. The covariate or pretreatment scores are located on the horizontal axis and the dependent variable or posttreatment scores are on the vertical axis. The ellipses represent the distribution of individual's joint scores for both groups. The regressions of Y_{ij} on X_{ij} for each group, or the prediction of the scores on the dependent variable for an individual with any given score on the covariate, are represented by the parallel regression slopes. Notice that for any given value of X_{ij} a higher value of the dependent variable would be predicted for members of the experimental group, Y_{ie} than for members of the control group, Y_{ic}. In other words, two individuals who are matched on the covariate (X_{ij}) have different predicted values for the dependent variable.

That difference, which is the treatment effect after the adjustment for the covariate, is represented here by the distance between the two parallel regression slopes. From Figure 1 it can readily be seen that the test for treatment effects is shifted from the differences $Y_E - Y_C$ on the marginal distribution of Y_{ij} to the difference in height between the two regression lines. Thus it is that one of the basic assumptions underlying ANCOVA is the regression coefficients β_{js} are homogeneous within treatments. When this is not the case, that is regression coefficents are not homogeneous, then ANCOVA estimates may be biased under certain conditions.

AN ILLUSTRATIVE EXAMPLE

The modified data presented here for illustrative purposes, were derived from a study examining the relationship between sex role identity and curriculum decisions for female junior high students (Moller, 1982). Despite major shifts in occupational opportunities for women, the early curricula decisions necessary for the pursuit of training in non-traditional careers continues to be problematic for young girls (Fox, 1977; 1981). One possible explanation has been that sex-role identity which is thought to be largely a product of environmental factors such as occupational stereotyping, is a major determinant of career planning (Moller, 1982). To that end, a pilot study was designed to examine the impact upon the sex role identity of ninth grade girls of a counseling program wherein the extent of sex bias in career choice was made explicit. The hypothesis was that sex role identity and thus ultimately career choices might be influencible if young girls understood the relationship between societal stereotypes and their career decisions. In the orginal design, subjects were three different math classes in a single school, but for practical reasons it became necessary to replace one class from a second school. Since the schools varied in geographic settings, as well as on a number of cultural dimensions, it was decided that a pretreatment measure of sex role identity scores should be obtained along with the primary dependent variable, posttreatment scores on the sex role identity scale. This allowed for the assessment of the impact of the counseling on the

sex role identity scores (Y_{ij}) independently of pre-existing group differences. For simplicity, the data from only seven subjects in each group are presented graphically in Figure 2.

Figure 2 About Here

It will be recalled that an analyses of variance of data obtained under conditions like that just described, is an analysis of the differences among the Y means unadjusted for the covariate, the pretreatment measure. These means shown on the Y axis of Figure 2 were numerically 24.43, 27.57 and 26.71 for groups 1, 2, and 3 respectively. The analysis of variance yielded an F = 12.40 which, with 2 and 18 degrees of freedom was significant at the .01 level. Apparently, the sex role identity scores were large for the two groups in school one. However, since subjects were not randomly assigned to groups, there is no reason to believe this difference is due solely to counseling; the groups may have been different prior to the treatment. Pretreatment scores were thus examined and the means for groups 1, 2, and 3 were found to be 22.14, 23.43 and 22.71 respectively. Moreover, from Figure 2 some linear correlation between the pretreatment and posttreatment scores is evident.

To complete the analysis, an ANCOVA was performed. One of the assumptions of Step 1 of an ANCOVA is that the regression coefficients, $\beta_{j's}$ are homogeneous within treatments permitting a common within-treatment regression slope. For these data, the separate slopes were .97, .70 and .60 and the common slope, observable in Figure 2, was .74. In the second step of an ANCOVA, an analysis of variance is used to test the significance of the difference among the adjusted Y measures. As shown for the general case in Figure 1 and for these data in Figure 2, these differences are represented by the difference in the heights for the regression lines. The adjusted means for the sex role identity scores were 24.89, 27.07 and 26.75 (F_{99} = 123.89, (2, 18) p $\leq$ .01). As can be seen, in this instance the ANCOVA and ANOVA results were in agreement. Differences between groups on the sex role identity scale are not due to pretreatment differences

in groups but may be interpreted as due to treatment. It should be noted, however, that adustment for the pretreatment score differences yielded a smaller error estimate (244.0 vs. 596.0 for the ANOVA) thus increasing the statistical power of the ANCOVA findings.

APPLICATIONS AND IMPLICATIONS

In the foregoing illustrative example, the analysis of covariance was successfully used to analyze data from non-equivalent intact groups. However, there are a number of important practical conditions of experimentation in field settings which can potentially cause bias in the ANCOVA results. Indeed, one of the major topics in Reichardt's (1979) wide-read discussion of the analyses of data from nonequivalent groups is the effect of bias. For example, several authors have argued that a covariate measured with error inevitably results in bias, in particular an under-adjustment in the estimation of treatment effects for the covariate. In other words, treatments would appear to be effective, when in fact they are not (Lord, 1960; Cook & Campbell, 1979). It is this argument, which has kept many researchers from using the analysis of covariance since almost no variables in the social sciences can be measured without appreciable error. Fortunately, Overall and Woodward (1977) have demonstrated in a series of Monte Carlo studies, that in the absence of other complications, the mere presence of error does not lead to biased estimates of treatment effects. It is true that the sampling variability of the parameter estimates increases, but bias is not introduced.

As both Lebow (1981) and Gray and Wandersman (1980) have pointed out in their respective reviews of outcome research in family therapy and home-based intervention studies, an ever present problem of intervention programs is the nonrandom assignment of subjects to treatments in such a way that covariate scores may be highly correlated with treatment assignment. This, of course, would introduce bias into the estimates of the treatment effects. For example, if a particular therapeutic approach is thought to be the most effective with clients who evidence a high degree of anxiety, then clients with high levels of observed anxiety would have a higher

probability of being assigned to the "treatment groups". Control group clients, those receiving a delayed or lesser effective treatment would be less likely to exhibit a high degree of anxiety. Beyond the ethical questions of withholding or using a treatment of lesser quality, there arises the statistical concern of the introduction of bias in the covariate or pretreatment measure. Overall and Woodward (1977) have again demonstrated that bias can be, but is not necessarily present under these conditions. For example, if group assignment is made on the basis of the true individual difference component of the dependent variable, that is assignment to treatments is not based on some sort of observed score, then ANCOVA will provide biased estimates of the treatment effect on the dependent variable alone. The proper estimate of the treatment effect, in this case, is the mean of the simple difference scores. In contrast as Overall and Woodward (1977) have shown, ANCOVA does provide unbiased estimates of the treatment effects when the nonrandom assignment is on the basis of an observed score. As in the earlier instance, here is yet another example of overinclusion (Reichardt, 1977) with regard to ANCOVA.

A more valid concern is the potential source of bias in ANCOVA estimates because of unequal group sizes. In a typical study reported by Avery, Haynes-Clements and Lumke (1981) on the evaluation of the effectiveness of a family life course for shy college students, individuals were assigned to treatment groups on the basis of class schedules thus resulting in fairly large group differences. Pretreatment differences among groups were controlled for with an ANCOVA. But, as Hamilton (1977) and Levy (1980) have shown, ANCOVA is not robust with respect to the assumption of equal group sizes with even small differences possibly resulting in biased estimates. However, given equal group sizes, Hamilton and Levy do demonstrate that ANCOVA is robust with respect to violations of equal regression slopes. The use of ANCOVA by Pinsker and Geoffrey (1981) in their study of the effectiveness of training and behavior modification of parent training thus seems quite appropriate. Although there were apparently large pretreatment differences within their subject groups, the equal groups sizes of 13, 13, & 14 appeared to counter any violation of the assumption of equal regression slopes.

Another fairly common practice among family researchers in the use of nonequivalent group designs is to replace ANCOVA with an ANOVA to

equate for pretreatment differences. The assumption being that nonsignificant results may be interperted as an absence of pretreatment differences. There are two difficulties with this approach. Primary is that ANOVA used in this manner in no way ensures that the experimenters have not made a Type II error. That is, a finding of no significant differences may or may not be valid, particularly given the rarity of concern for Type II error by most researchers. Second, whatever increase in power that may have been gained from the use of ANCOVA is lost. This latter is of particular concern with the small samples common to intervention research. Hence, for example, in the outcome study of behavior marital therapy and communication therapy. O'Leary and Turkewitz (1981) may well have been able to increase both the power and validity of their results if ANCOVA had been used in place of an ANOVA of pretreatment group difference.

The last application of ANCOVA which is of interest to family researchers to be discussed here is what has come to be referred to as the "fan spread hypothesis" (Kenny, 1975). Basically, the fan spread hypothesis states that initial group differences will continue over time to become larger without treatment. For example, if the differences in physical measurements were plotted for groups of girls and boys over time the resulting graph would have a fan shape. Independently of treatment, observed physical differences between boys and girls will continue to become larger as they grow older. An almost classical example of the fan spread phenomenon is to be found in an article by Parcel and Luttmann (1981) reporting the evaluation of a voluntary sex education course for eight graders. On page 58 of that report is a plot of the attendence by levels of knowledge. Not surprising those students with the best attendance records scored highest on the knowledge tests. But careful examination of the data also indicate something else of great importance. These same students (high attendence, high knowledge scores) also had the highest initial scores on the knowledge test and those students with the lowest posttreatment scores had the lowest pretreatment knowledge scores. Simply, until these pretreatment differences among attendance groups have been controlled for, the results reported by Parcel and Luttmann are in question, in that there is no method by which the observed differences in the

dependent measure can be attributed to treatments alone.

In summary, the principal objective of this paper has been to bring to the attention of family researchers the utility of ANCOVA as a statistical control in a broad range of research settings. With the increased emphases on both field research and the evaluation of field research, statistical control techniques have assumed new importance for family researchers. It is not the intent here to "sell" ANCOVA as a panacea, but to demonstrate that ANCOVA is an appropriate technique under a fairly broad range of conditions. For this reason the emphasis was placed upon the basic model to the exclusion of extensions of ANCOVA to its more complex uses with multivariate or repeated measures designs.

FOOTNOTES

1. Intervention as used here includes intervention in clinical/therapeutic settings as well as nonclinical settings.

2. For example, standardized gain scores analysis, raw gain score analysis and analysis of varience with blocking or matching. See Reichardt (1979) for a relatively complete discussion of these approaches.

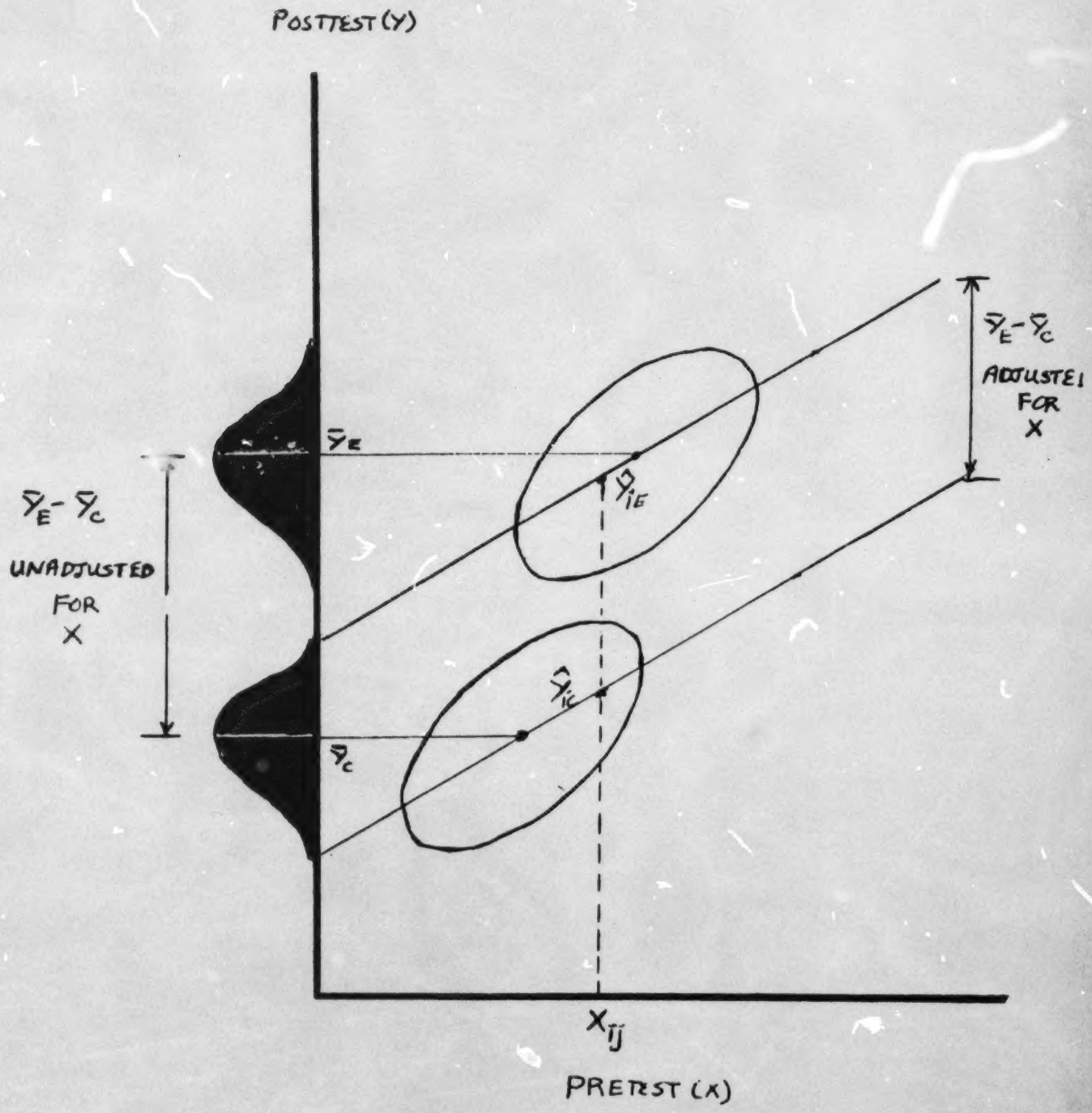
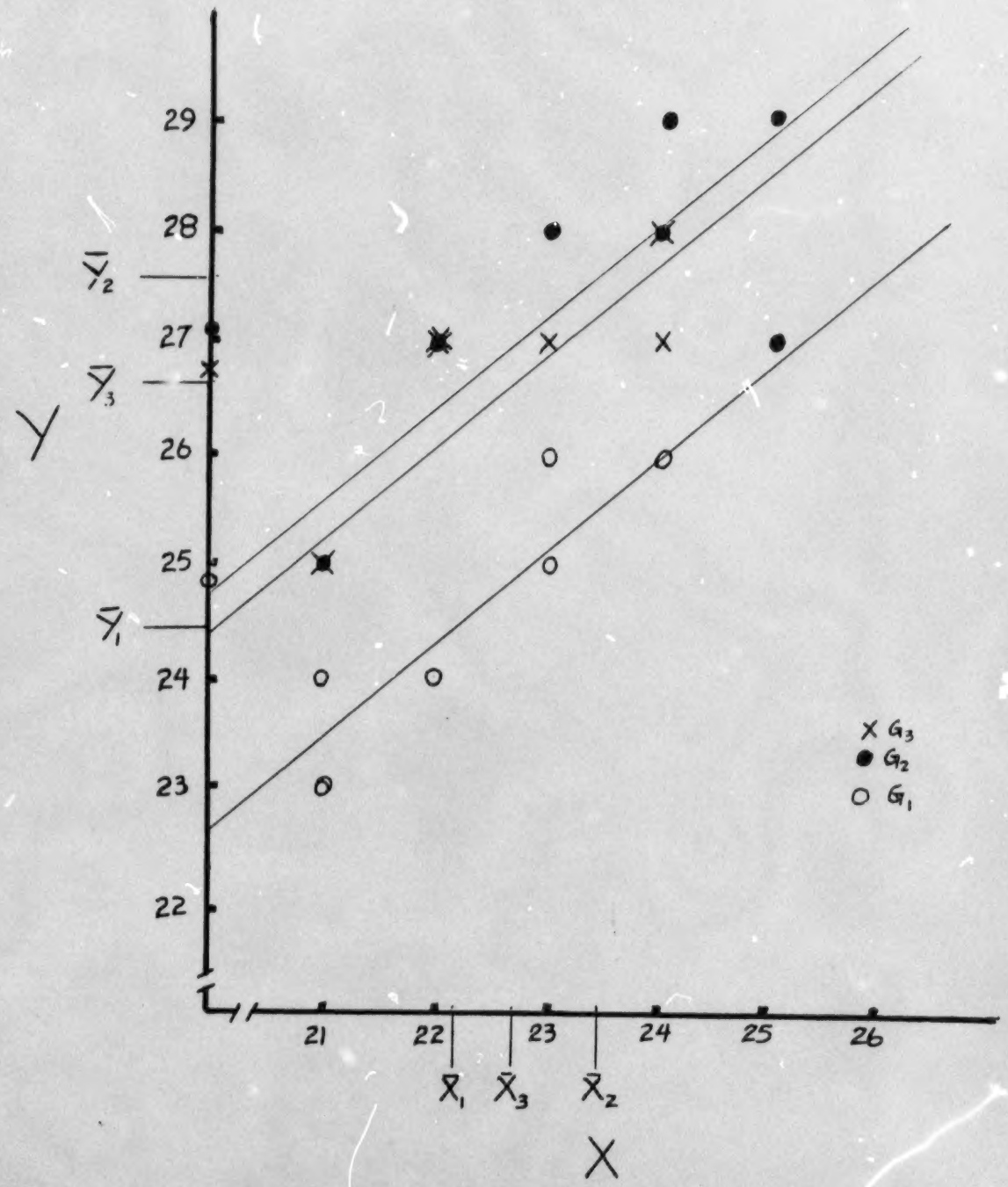


Figure 1

Plot of Pretreatment VERSUS Postreatment Scores for Analysis of Covarience (Adopted from Reichardt, 1979)

Figure 2

Plot of ANCOVA Results for Seven Subjects in Groups 1,2,3



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